Letters to the Editor.

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Amplifying the Optophone.

IT may be of interest to record some experiments that I have recently been making on the application of a thermionic amplifier to increase the volume of the sounds produced by Dr. Fournier d'Albe's very wonderful optophone so as to render these sounds audible to everyone in a room without the necessity of each listener being furnished with a separate telephone receiver.

The experiments were carried out at the instance of Mr. J. M. McCarthey, who is teaching blind soldiers to read with this instrument, and who asked me whether it would not be possible to magnify the sounds sufficiently to enable a class of a dozen or more to hear them simultaneously.

The Fournier d'Albe optophone instrument employed was one of the improved type designed and manufactured by Messrs. Barr and Stroud, and the amplifier I found to work best out of several I tried was an audio-frequency one with three "R" valves, transformer-coupled, of the French military type. This was used with a Brown loud-speaking telephone with considerable success.

In Mr. McCarthey's opinion, and so far as a person such as myself, who has no experience with the optophone, could judge, the best results were obtained when the optophone was arranged for what is technically known as "black sounding," when the white paper is represented by silence and notes are sounded as the beam of light passes over the black letters.

I have very little doubt that still better results could be obtained with an amplifier specially designed for the purpose. Further experiment is desirable in order to obtain the best results, but, so far, what has been accomplished is quite encouraging.

A. A. CAMPBELL SWINTON.

66 Victoria Street, London, S.W.1,
February 25.

Molecular and Cosmical Magnetism.

DR. CHAPMAN'S important letter (NATURE, November 25, 1920) bases a theory of cosmical magnetism on the presence of gyroscopic magnetic elements proved to exist in ferro-magnetic substances by my investigations on magnetisation by rotation. But he considers my fundamental theory to require serious modification. As I understand his letter, however, his theory is identical with mine (see *Science*, vol. xlviii., p. 304, 1918, and references) except as to paramagnetic and diamagnetic bodies. He has, I think, confused my treatments of magnetic intensity and intensity of magnetisation.

While in my papers electron rings or orbits have been assumed, the fundamental theory is essentially the same if ring electrons or magnetons of other types, preferable for Dr. Chapman's purpose, are assumed instead; and I have referred to this equivalence before the Physical Society and elsewhere.

The gist of the theory is this: A magneton or electron orbit, being a gyroscope, tends to take an orientation with the direction of its revolution coincident with that of any rotation impressed upon it. Being a magnet, it also tends to set with its axis parallel to an impressed magnetic intensity. Ultimate coincidence in either case may be prevented by extraneous forcives. But, in given circumstances, whatever the forcive towards alignment, and whatever alignment of the magneton is produced by a magnetic intensity, H will be produced by rotation about the direction of the intensity with velocity $\Omega = H/R$, where R is the ratio of the angular momentum of the magneton to its magnetic moment. The general idea has been applied to cosmical magnetism by Schuster (1912), by Einstein and by de Haas (1915), and by myself (1909 and 1915), though not with Dr. Chapman's detail.

If all the magnetons within a body are alike, rotating it at velocity Ω will produce the same magnetisation as would be produced by applying a uniform magnetic field of strength $H = R\Omega$.

For weak fields the ferro-magnetic bodies rotated all receive intensities of magnetisation proportional to the intensities of the fields applied, and are thus *magnetised* by rotation proportionally to velocity. This proportionality exists only for elastic displacements to which Dr. Chapman refers (and to which I have referred, comparing the molecular forces to those due to springs).

If the magnetons in a body are of two kinds, positive and negative, with constants R_1 and R_2, rotating the body will have the same effect as if a magnetic intensity $H_1 = R_1\Omega$ were applied to the positive magnetons and an intensity $H_2 = R_2\Omega$ were applied to the negative magnetons. If the effect on the negative magnetons is preponderant, the rotation will thus produce an intensity of magnetisation in the direction of H_2, but of magnitude less than that which would be produced by the intensity $R_2\Omega$ if all the magnetons were negative.

When the displacements are not elastic my theory gives results analogous to those of Voigt for a swarm of magnetons in an ordinary magnetic field. If there are N similar magnetons per unit volume, if the rotations are damped only about the axes perpendicular to the magnetic axis, and if the effects of collisions and the molecular field are negligible, all the magnetons, even in the weakest magnetic field of strength H, will ultimately become oriented with their axes in the direction of the field. In this case, if C and U denote the moment of inertia and initial (permanent and undamped) angular velocity about the magnetic axis of a magneton, the intensity of magnetisation will be

$$I = NC/R.(U - H/R).$$

The first and principal term is entirely independent of H. The orientation is produced by the field, but only the time taken to arrive at the steady state is affected by its magnitude. If collisions are not absent, or the molecular field becomes appreciable, the intensity of magnetisation will not reach saturation, but will increase with the field strength, being greater for a given applied field strength the greater the time between collisions and the weaker the molecular and demagnetising fields.

For the same swarm of magnetons subjected to an angular velocity Ω instead of a magnetic field with intensity H, we have, when the effects of collisions and the molecular and demagnetising fields are negligible,

$$I = NC/R.(U + \Omega).$$

The first and only important term is independent of Ω. Here the orientation is produced by the velocity

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impressed, but only the time taken to reach the steady state is affected by its magnitude. The effects of collisions and of molecular and demagnetising fields are essentially the same as in Voigt's case.

Like Dr. Chapman and others, I have considered the possibility of dissociations increasing the intensity of magnetisation of hot bodies, and I have plans for experiments in this field. If the gyroscopic behaviour of a magneton is to account for cosmical magnetism (and it was the contemplation of this which led me to the rotation experiments), we must, as has long been evident, assume a constitution of the earth and sun different from that of materials on which experiments have previously been made.

On my theory, a magneton in a diamagnetic or paramagnetic body set into rotation is acted upon by the same alignment forcive as if alone or in a ferromagnetic body. But the intensity of magnetisation in the latter is small, for the same reason for which it is small when the body is placed in an ordinary magnetic field. In the former it is zero, because, on the assumption I have made, with Weber and Langevin, the magnetons are grouped rigidly together so that no element with a magnetic moment can have its orientation changed. This is the only point on which Dr. Chapman's theory, as I understand it, differs from mine. Rotation experiments on diamagnetic and paramagnetic bodies by Lébedew and by Mrs. Barnett and myself have hitherto given no magnetisation.

S. J. BARNETT.

Washington, D.C., January 31.

I FULLY agree with Prof. Barnett's statement of the theory of magnetisation by rotation, and regret that through misunderstanding his treatment of magnetic intensity I suggested that his theory required modification. I am glad to know that he contemplates experiments on the rotation of hot bodies; this point, and the greater possibilities afforded if the magnetic elements remain intact at high temperatures, are the matters to which chiefly I wished to direct attention. Experiments made here with Dr. Oxley have negatived my suggestion that diamagnetic and paramagnetic bodies should also show magnetisation on rotation, thus confirming the previous results mentioned by Prof. Barnett; experiments on hot ferro-magnetic bodies are not yet advanced sufficiently to state whether they support the view that the earth's magnetism may depend on its high internal temperature. Further trial seems to preclude the possibility of trustworthy calculation at present, and the view must be tested by experiment. Until this is done it seems useless to enter into further details of the earth's field and its secular variation.

As regards the sun, later consideration of the narrow radial limitation of its magnetic field leads me to think that no simple magnetisation, by gyroscopic action or otherwise, is the probable cause; any such view requires two hypotheses, one to explain the production and the other the neutralisation of the field. A unitary hypothesis, such as the second of those indicated by Sir J. Larmor in the British Association Report for 1919, seems preferable.

S. CHAPMAN.

The University, Manchester, February 22.

Transcendental Premises in Science.

PERHAPS you will permit one who belongs to a considerable section of your readers who are neither mathematicians nor neo-physicists to state how the very remarkable discussion on Prof. Einstein's theory in NATURE of February 17 appears to some of us.

Mathematics to us is a very precise and complete form of deductive logic applied to space and number. It differs from ordinary logic only in having its arguments set out in a symbolical shorthand instead of in words, and thus enables a long deduction to be condensed into a short statement. This unfamiliar form of notation and condensation of the argument are the chief stumbling-blocks to the outsider.

Like other forms of logic, it is an art rather than a science, namely, the art of drawing legitimate conclusions from premises. In essence, it has nothing to do with the truth or falsity of the results. These depend entirely on the nature of the premises. The most faultless string of equations, like the most immaculate collection of syllogisms, may conclude with an absurdity or a stupendous error if the premises are faulty. The logical mill by which the results are obtained may turn out good flour or only chaff. This depends entirely on what it is fed with.

This is why the Philistine who is not a mathematician sometimes shakes his head when he is presented with a series of equations on the blackboard and his teacher says to him: "Look there. What do you say to that?"

What the Philistine doubts is not the accuracy of the deduction in this case, but the validity of the premises used in the new departure, which turns largely on the nature of space and time as defined by the neo-physicists. Granting that they are legitimate, the results are unquestionable. Are they legitimate? Let us turn to space. The first remark I would make is that, whatever its value, the definition in question represents something entirely and confessedly different from space as known to the great mass of men and to all philosophers, mathematicians, and physicists until the last few decades, and it has, therefore, no claim to be called space at all.

Space was defined by Newton by two predicates, namely, extension and immovability. I would presume to add a third one, quite necessary as things are now marching, namely, that any finite portion of space may be measured by three co-ordinates at right angles to each other and passing through one point—or, in other words, space has three dimensions. This is the only space known to human experience, as it was to the early geometers. The addition of a fourth or any number of other dimensions as factors of space is inconceivable unless we entirely alter the comprehension and connotation of the words "space" and "dimension." You may call the result what else you will; you are misleading a great many innocent people in calling it "space," like the Pragmatist is doing when he defines the "truth" he writes about as "the useful."

When Riemann read his famous paper before the Göttingen Academy at the instance of Gauss, who presided on the occasion, he first introduced the notion of space with more dimensions than three. He spoke entirely as a pure mathematician. His premises were not facts, but definitions of abstractions which could not materialise into realities. With his abstract postulates he was able to frame a series of equations which were quite legitimate in form, but the conclusions of which were also abstractions, and could not be presented in a mental picture or as representing anything in Nature. Since then, a large literature has grown up in regard to these phantasms of mathematical abstraction. Attempts—very futile attempts, as it seems to me—have been made to translate the conclusions of Riemann's equations into

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